Comments to both reviewers:

We are grateful to the reviewers for their careful reading of our manuscript and their many helpful comments. We have tried to address as many of these comments as possible and the revised manuscript is greatly improved as a consequence.

Both reviewers raised questions regarding the physical validity of the SCR64 parameterization, questions that frankly we had not intended to address in this paper since our principal goal was to examine the impact of a change in modeled emission height on energy derivations. However, upon re-reading the manuscript it is clear that we invited these questions from the way the paper was structured and the extended discussion given at the end of the paper on the validity of the SCR64 parameterization.

Clearly this single modification is not appropriate for aeronomic calculations and more work would need to be done if our goal were to present a more accurate cross section parameterization. Both reviewers suggested this course of action. However, our goal is much more limited, namely the energy derivation sensitivity study mentioned above.

With this point in mind, and with multiple requests for a shorter paper, we have removed much of the end discussion along with several of the figures we felt we could spare. In addition, we have tried to refocus the paper on our goals and frankly admit the shortcomings of the cross section modification used.

However, these shortcomings do not limit the validity of the study we've performed here, a point we feel strongly about and have tried to emphasize in the revised paper.

Detailed replies to the individual reviewer's comments are given below, interspersed in the comments and contained within square brackets, [like this].

Referee #1 Review of "Impact of model differences in quantitative analysis of FUV auroral emissions: Total ionization cross sections"
Authors: Germany, et al.
Manuscript Number: 20.0354
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In your opinion, does this paper describe interesting and substantial new results? If yes, briefly describe their nature and potential impact.
Yes.
****

In your opinion, does this paper adequately put the progress it reports in the context of previous work? (This includes both representative referencing as well as introductory discussion.)
Yes.
****

Is the paper clearly and concisely written? (Note it is not necessary to include every detail to be "clear".)
Yes, but should be shorter.
****

Are the conclusions and potential impact of the paper made clear? If no, how can the author make them clearer?
Yes, with some exceptions as noted below.
****
Will readers outside of the specialty of this paper be able to appreciate at least the motivations and general conclusions of the reported work?
Yes.
****
Is the paper up to JGR standards in terms of:
Writing style: Yes
Graphics: Yes
Professionalism: Yes
Apparent Accuracy: Yes

****
Summary Evaluation
Reviewer recommendation:
Recommend revising
****

Additional comments:

This paper shows the results of sensitivity studies using different N2 ionization cross sections in an auroral electron transport model. The point is made that ionization cross sections may be more uncertain than is generally thought, and that this can have a significant effect on model predictions, and hence on energetic electron characteristics inferred from auroral images. This is a valid point, but the length of the paper devoted to making it is disproportionate to its size. One recommendation is thus that this paper be drastically shortened, as it contains a large amount of extraneous material and is much too long in comparison to the new information conveyed. The text should be greatly reduced, and figures other than 3, 8, 11, 13, and possibly 14 should be removed. In addition, there is a substantial issue with the validity of the cross section parameterizations employed. Neither the "SCR64" nor the "GRL00" parameterizations are in particularly good agreement with data, theory, or other parameterizations, although it is agreed that they do "bound" the problem as extreme cases. The SCR64 parameterization has particular problems, apparently due to the assumed high-energy power-law slope. Thus, the authors do not recommend its use. A suggestion would then be that it be modified. An additional point - although the ionization cross section affects the inferred characteristic energy, it does not affect the peak height inferred from FUV measurements using the differential absorption technique.

[The number of figures has been reduced from 14 to 6 and much of the end discussion has been omitted.]

P. 2: The two first sentences are vague - the specific modeling uncertainties should be described in the topic sentences.

[the abstract has been rewritten to address this point.]

P. 2: Changes of up to 3 km in what?

[the abstract has been rewritten to address this point.]

P. 2: Why 15 keV? Above and elsewhere, 10 keV electrons are considered. 15 keV is an unusually high auroral mean energy - why use it as an abstract benchmark?

[15 keV was chosen because it represented the maximum changes seen in our modeling. however, we agree that a 10 keV benchmark is more appropriate and have changed the text accordingly.]

P. 3: A brief explanation of the O2 differential absorption method should be provided here (such as from p. 5).

[this has been added to the end of the first paragraph.]

P. 4: Without affecting the model atmosphere?

[the choice of cross section doesn’t directly influence the choice of model atmosphere.]

P. 8: "...their cross section includes other ionization products..." - isn't it the other way around?

[correct. the text has been changed.]

P. 8: "...For this reason..." - what reason?

[the text has been rewritten to make this more clear.]

P. 9: "The error bar...from table 1 of Van Zyl and Pendleton..." - These results should be summarized, either with a similar table, or a text giving, e.g., the number and range of measurements. The implication is that there is reasonable agreement among peak cross section measurements since Rapp and Englander-Golden - is that correct?

[correct. the numeric range of the error bar has been added to the text, as has the number of studies involved and the time span of the studies.]

P. 9: "35% at 10 keV" - It looks closer at 7 keV. Could the 10 keV point be anomalous?

[actually, we think the 7 keV value may be too high. the problem is that the plotted symbols were estimated from a figure from an old paper--numeric data was not available. the figure was reexamined and the 7 keV value was revised downward ~2.5%. we've added a caveat to the text explaining the origins of these points.]

P. 9: "The GLR00 extrapolation is based, in part, on...Borst and Zipf" - But the high-energy power-law slope appears to be different. If Borst and Zipf extrapolated their data using a Bethe-Oppenheimer relation, then is it the claim of GLR00 that this relation is incorrect?

[On the contrary. The original RT90 (and GLR00) parameterization used the Bethe-Oppenheimer relation Q = A ln(BE)/E taken from Borst & Zipf. This relation was fit to the Rapp & Englander-Golden data to determine the fit parameters A and B in the equation. Differences in the high energy behavior stem from differences the fitted data sets. For example, Borst & Zipf were able to fit data up to 3 keV, while we had Rapp & Englander-Golden only up to 1 keV. This has been clarified in the text.]

P. 10: For SCR64, the parameterization is even further off from the slope of either Strickland and Meier or Borst and Zipf. The slopes of these latter two appear to be in approximate agreement. However, SCR64 is even lower than the Schram et al. measurements. Aside from the 10 keV point, the slope of Schram et al. appears to be in reasonable agreement with Strickland and Meier and with Borst and Zipf, i.e., with Bethe-Oppenheimer theory. So the same question can be asked of SCR64 as of GLR00 - what is the argument that either of these are likely to be correct, if they are not supported by either data or theory? A possible reading of figure 3 would indicate that Schram et al. are not greatly in disagreement with either Rapp and Englander-Golden or Strickland and Meier, other than a small offset that is within the range of the indicated error bar. But adopting parameterizations that change the slope magnify differences when it is extrapolated out to 10 keV.

[We disagree that the GLR00 cross section is not supported by either data or theory. As noted above, it is derived by fitting the Bethe-Oppenheimer relation to the Rapp & Englander-Golden data. We also noted above that we believe there was a plotting error in the 7 keV point of the Schram data and that the SCR64 parameterization reasonably matches the values, if not the slope of the Schram data.

More importantly, this comment underscores the rationale for this study, namely, that there are unavoidable uncertainties in selecting the cross sections for use

in our models. The reviewer rightly notes that these uncertainties are magnified with the high energy extrapolation. Our goal here is to estimate the impact of these uncertainties, rather than trying to arrive at a ‘correct’ cross section.]

P. 12: Figures 5 and 6 have insufficient resolution for ratio plots (although, as noted above, they are unnecessary anyway).

[the figures have been removed]

P. 14 and Table 1: Since the cross section is unchanged at 500 eV, it is not surprising that the ratios are also unchanged.

[the 500 eV values have been removed]

P. 17: Use of percentages in various senses ("70% of that value"; "reduced by 30%" is confusing. Percentage reductions should be avoided as potentially ambiguous, but if used, should at least be consistent.

[agreed. we’ve tried to remove the worst offenders.]

P. 18: The 3371/3914 ratio is not a commonly used diagnostic of auroral energies, for the reasons discussed here and in the papers referenced.

[this section has been removed.]

P. 19: Observations of the N2 2P to N2+ 1N ratio from space [Solomon, JGR, 1989] are also germane to this discussion.

[agreed. however, this section has been removed.]

P. 19: The probable reason that the SCR64 parameterization is not constant at higher energy, contradicting both measurements and other models, is that this parameterization changes the high-energy slope of the N2+ cross section with out changing other ionization cross sections. Again, this begs the question of why the SCR64 parameterization (as opposed to the actual Schram et al. measurements) should be considered. If there is some reason

why this behavior of the energy dependence is correct for the N2 molecule, it should also be true for, e.g., the O2 molecule.

[please see comments above.]

P. 20: "Thus, the SCR parameterization presented here is clearly inappropriate for modeling purposes."

[we believe the reviewer is pointing out the irony of saying the cross section we've based our paper is inappropriate for modeling. what we meant is that the artificial modification we've included for the purpose of this study is not appropriate for other modeling purposes. (see comments above.) we've changed the text to emphasize this.]

P. 21: "the perturbation used here is unrealistically large" - Perhaps a more realistic perturbation, e.g., one scales the Strickland and Meier parameterization (or, e.g., Jackman et al. 1977) to better fit the Schram et al. measurements, should be used instead.

[we agree. however, please see comments above.]

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Referee #2 Review of MS# 20.0354 by Germany et al

This manuscript is a rare example of modelers performing quantitative calculations to ascertain uncertainties in the prediction of model results due to specific changes in one input parameter. The paper serves as an example to the modeling community that should be adopted more widely.

I have five comments and questions that the authors might address, and a few corrections of grammar and sentence structure.

1)If a coding error in the RT90 model accounts for a 12% overestimate in the cross section why use this model as the 'reference' as is done in Fig. 12. Do the percent reductions quoted in the Abstract and elsewhere refer to the RT90 or the GLR00 cross sections?. It is important, of course, to mention that the coding error had little impact on previously published results.

[we agree and have changed our reference to GLR00. in addition, we've only limited the number of RT90 comparisons.]

2) A user of electron energy parameters (flux and mean energy) derived from modeling UVI images would like to know how the uncertainties resulting from the N2 total ionisation cross section compare with uncertainties in instrument calibration, partial filling of field of view, temporal changes, etc.

[we've added a discussion of this.]

3) p.19 and Fig.14 Since the two lines marked (c) differ significantly the reader would like to know what 'two cross section sets' were used in the Lummerzheim and Lillensten (1994) model calculations. Was the N2 total ionisation cross section one of the cross sections, and were there others?

[the figure and discussion have been removed.]

3)p.19 It would be more informative if the energy scale in Figs.2 and 10 through 14 were logarithmic instead of linear, as given in Figs 3 through 6

[we redid the plots as requested. however, there is no appreciable low-energy structure (our lowest modeled energy was 100 eV) and we felt the original presentation was more useful for our purpose.]

4). What is the explanation for the departure from a constant 3371/3914 ratio below about 5 keV? (present model results). Previous model results and observational data show a constant ratio (e.g. Strickland et al, 1989; Hecht et al 1995; Sharp et al, 1979) What does this say about the role of the N2 total ionisation cross section for modeling this ratio, given that a large fraction of electron precipitation events have mean energies below 5 keV?

[we have removed this figure and discussion from the paper. we acknowledge an excellent question for which we don't have an answer at present. the implication is that this ratio is sensitive to this ratio. conversely, the assumption of a constant ratio could be used to constrain future cross section modifications.]

A few minor items:

Abstract, line 3: insert the word 'profiles' after altitude.

[done]

Abstract, line 10: delete the word 'more' preceding different.

[done]

p.4, line3: affect, not effect.

[done]

p.6, 3rd para. line 4: there are no 10 keV auroral emissions;
change to …..the region where auroral emissions produced by 10 keV electrons peak.

[done]

p.7, line 7: after excitation insert 'dissociation'.

[this paragraph has been deleted.]